hotel dining rooms previously each served by an independent kitchen and chef, appears on paper attractive. The practical solutions rest on the results of detailed study and costing. So the technical side of catering seems on the edge of imminent dramatic changes. But equally the same may be true of domestic meal preparation. The food scientist, food technologist, caterer, housewife and all consumers will, by their responses to what is to be attempted, determine future developments. These developments will in their turn react on social life in may ways, which only the next twenty years will reveal.

The author is indebted to Mr. G. Glew, director of the Catering Research Unit, Procter Department of Food and Leather Science, University of Leeds, and his staff for many discussions which have given rise to this paper.

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Why industrial innovations fail

Innovations do not normally fail because of sophisticated technical problems which cannot be overcome. Nor do they fail primarily because of factors like the size of the firm or how much competitive pressure it faces. They fail for much simpler reasons, which usually reflect the fact that well-known principles of innovation management are not applied.

This is the main general conclusion of *Success and failure in industrial innovation*, a report published recently on the SAPPHO Project undertaken at the Science Policy Research Unit of the University of Sussex over the last three years. The report, published by the Centre for the Study of Industrial Innovation, 162 Regent Street, London W1R 6DD, price 75*p*, is a short version of the full two-volume report on the Project which was presented to the Science Research Council at the end of 1971.

The results of the SAPPHO Project are based upon a unique analysis of industrial innovation. In most industries where innovation is crucial – SAPPHO examined the chemical and scientific instrument industries – new products are usually soon subject to competition from rivals, but not all of these competing products achieve commercial success. The method employed in the Project was to pair together competing products, one of which succeeded and one of which failed. These were then compared in detail, using 122 different measures, to isolate the characteristics which differentiated between the products.

The findings of the SAPPHO Project demonstrate that there is little support for beliefs in 'single factor' explanations of innovative success. For example, size of firm does not in itself appear to be a factor determining the success or failure of an innovation. Nor – to quote some other common views – does innovative success appear to depend very much upon the degree to which the company is under competitive pressure in the market, or how willing it is to take risks, or even on its degree of familiarity with the technology involved.

The factors which were found to be most important in differentiating successful new products from failures included the following:—

1 Successful innovating firms displayed a better grasp of user needs. The less successful ones tended to ignore user requirements and even users' views when offered.
2 Successful innovating firms were better at marketing. The failed innovations were characterised by inadequate market research and neglect of publicity and user education.
3 Successful innovating firms performed technical development more thoroughly than the less successful, though not necessarily more quickly. They tended to eliminate technical defects before launching on the market.
4 The successful innovating firms made more use of outside scientific and technical advice, and maintained more effective links with the scientific community in the technical area specific to the innovation.
5 In successful firms, the key managers involved in a new product had a higher rank and more authority than those responsible in the less successful firms.

The report points out that the SAPPHO results do not constitute a recipe for automatically successful innovation. Some of the factors it discusses are not easily subject to control within the firm. But the authors conclude that to understand the relationships between the different factors which influence success or failure could help to make failure less frequent (or, when it does occur, less costly), and may help to reduce the possibility of failure even if it cannot guarantee success.

Letter to the editor

Scientists outside science

Sir, – P. C. Smethurst's letter[1] is both penetrating and superficial. He confuses scientific laws and method and his views give a static picture of events.

The implication when a science graduate is obliged to look for a 'professional' job in another field, is that he or she would eventually see aspects, as probably would the employer, to which scientific method could be applied. Otherwise how else have scientific activities penetrated into new areas and the areas of present technology that they have.

Science graduates and scientists are not 'clueless' in practical matters which arise in everyday life; they have ability to appreciate the process of classification, an activity fairly prevalent in daily life. They are able to appreciate concepts. They can discern the principles of an operation and are used to starting enquiries from first principles which is quite useful if one is operating in another line of activity. They see events in quantitative as well as qualitative terms and they are used to the idea of devising models. None of these are without some application in all human activities quite apart from scientific use.

It is irrelevant to the question in hand to mention any application of scientific laws to human behaviour. I would not have thought that there was a conflict between human emotions and science – scientists are as appreciative as any of art, music, literature and politics and frequently participate in these things – but between an attachment to the logical which may stifle the imaginative approach.

Finally I think P. C. Smethurst is out of touch with the versatile attitude and approach of the present day science graduate. All arts graduates do not expect to obtain jobs concerned with history, classics, politics and economics, so why should science graduates always work inside science?

Yours faithfully
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Reference
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Patterns of innovation

Part III - The bipyridyl herbicides*

F R Bradbury, M C McCarthy and C W Suckling

Hormone weedkillers

The bipyridyl herbicides, paraquat ('Gramoxone') and diquat ('Reglone'), possess a unique pattern of end effects. They are rapid acting, non-persistent and non-selective; because both are basic, they are rendered inactive almost instantaneously when they contact soil; they are readily adsorbed to plant surfaces, and consequently may be applied during rainfall without reduced effectiveness.

To appreciate their properties, it is important to compare paraquat and diquat's features with those of other newly developed herbicides. The use of organic chemicals as herbicides is relatively recent, for it was not until 1932 that the first organic weedkiller, DNOC (2-methyl-4,6-dinitrophenol) was introduced. Indeed, herbicides themselves were of relatively small importance until the introduction in 1946 of the 'hormone' weedkillers, 2,4-D (2,4-dichlorophenoxy acetic acid) and MCPA (2-methyl-4-chlorophenoxy acetic acid). MCPA and 2,4-D ushered in a new era in chemical weed control, with rapid and great increases in the land area treated. This was due to the variety of properties possessed by these weedkillers: they were potent enough to be used in low volumes; as contact weedkillers applied to plant foliage they killed perennial weeds; applied to the soil, either before or after the preferred crop had emerged (pre- and post-emergent), they were absorbed by weed roots; and they were selective against many broad-leaved weeds in cereals (selectivity is used to denote the ability to kill unwanted plants without harming the preferred crop). Their mode of action was manifested through a change in growth rates, and by analogy with certain plant hormones which control growth rates they received the name 'hormone' weedkillers.

The impact of hormone weedkillers was not confined to those who used them, for their success strongly influenced those who defined research targets for new herbicides. This influence may be seen in events at Plant Protection Ltd, where MCPA had been discovered and where in due course both diquat and paraquat were to be recognised as potential herbicides. In 1958 a market survey estimated demand for a replacement for, or improved form of, hormone weedkiller to be six times as large as that for any other type of herbicide, and to comprise more than half the total demand for herbicides. Consequently, great importance was attached to discovering chemical compounds with the properties of hormone weedkillers – that is, a herbicide possessing selective activity, applicable to either foliage or soil. This survey reinforced the decision taken in 1950 and 1953 to introduce new screens designed to measure the growth-altering reaction of plants to 2,4-D and MCPA, to test compounds for selectivity, and to test whether any compound possessed pre-emergent activity. The overall target, and the screens used to test candidate compounds, were both defined with reference to the hormone weedkillers. Plant Protection's concentration on this type of herbicide was reflected in contemporary reviews of herbicidal research,[1] which emphasised greater selectivity as a prime target.

Recognition of a new herbicide

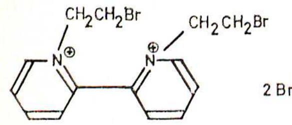
It was in a framework of seeking after selective herbicidal properties, and in an organisation whose professed objective and actual testing facilities stressed the importance of chemicals possessing these properties, that the bipyridyls were discovered. Plant Protection Ltd had been established in 1937 to develop the crop protection interests of ICI Ltd and of Cooper, McDougall and Robertson Ltd. Before the bipyridyls it had two considerable successes, the discoveries of the hormone weedkiller MCPA already mentioned and of the insecticide BHC. The exploitation of both these discoveries suffered from wartime restrictions.

The original observation to which the bipyridyl herbicides may be directly traced occurred in 1947. It was noted, in the course of trials to establish the effectiveness of a compound formulated with a wetter, that the wetter itself was

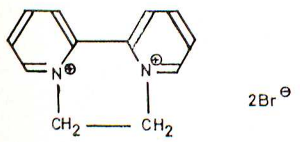
* Part I appeared in *Chemistry & Industry*, 1 January 1972, p 22 and Part II in the issue of 5 February, 1972, p 105

phytotoxic. The wetter – dodecyltrimethyl ammonium bromide – was believed to act as a detergent, scouring wax from plant leaves, and to this was attributed a characteristic scorch that was observed. The hypothesis was advanced that quaternary ammonium salts with one long and three short carbon chains attached to the nitrogen atom would be the most powerful scorchers, and therefore the best weedkillers. Various quaternary ammonium salts, many synthesised for purposes unconnected with their use as potential weedkillers, were consequently collected from several parts of ICI and tested. In 1954, the specimen collection of Organics Division at Blackley was examined for quaternary ammonium salts, and a selection was sent for testing at Jealotts Hill, Plant Protection's research station. One compound – specimen K8483 – was tested in February 1955, and proved very active when applied at 5 lb/acre. Subsequent tests showed activity at application rates lower than 1 lb/acre.

K8483 had originally been synthesised in December 1950 by R. J. Fielden of Organics Division – in the course of work that was not aimed at synthesising biologically active compounds – and he had assigned it the structural formula:



But on reinvestigation, it was found to be N,N′-ethylene-2,2′-dipyridinium dibromide:



This compound, subsequently to be known as diquat, was the first of the bipyridyls to be discovered. Its discovery presented at least three problems: first, by what mechanism were the compound's herbicidal properties operating? An understanding of this mechanism would be very helpful in exploring other closely related compounds on the outcome of which would turn the decision to develop diquat or to wait for an improved herbicide. Second, were the properties possessed by diquat compatible with the target of a selective herbicide? And, third, could the compound be economically manufactured? Originally, in two preparations each lasting eight hours, Fielden had synthesised just over 18g.

The mechanism of the bipyridyls' action

To elucidate the mechanism by which diquat exerted its phytotoxicity, it was necessary to synthesise other bipyridyl quaternaries. After observing the herbicidal activity of various bipyridyl salts, it was possible to relate the phytotoxicity of compounds to their chemical structures. It was first noted that all active compounds were extremely rapid in their effect. So fast an onset of necrosis could not be due to metabolic disturbance. To explain this phenomenon, the suggestion was advanced that necrosis was caused by cell membranes being ruptured by free radicals produced by the bipyridylium salts. The biological work was accompanied by a study of the physical chemistry of the compounds. It had been known since 1933 that the 4,4′-bipyridylium could be reduced to a free radical of unusual stability.[2] This had been attributed to resonance in the molecule, and, as is well known, delocalisation of an odd electron over the molecule is greatly facilitated by coplanarity. Since the posited explanation of the bipyridyls' herbicidal activity emphasised the part played by free radicals, this directed chemists' attention to those structures which might be expected to yield free radicals. An initial hypothesis was advanced that bipyridyls which were coplanar would possess phytotoxicity.

This hypothesis was supported by observing the different phytotoxicity of several similar compounds. It was found that 2,2′- and 4,4′-bipyridyls, both of which were coplanar, were both more active than 2,4′-bipyridyl, when steric hindrance prevented coplanarity. Particularly, the phytotoxicity of the 2,2′ diquat, where the nitrogen atoms were linked by a chain of two carbon atoms, was found to be sensitive to the molecule remaining coplanar. If the coplanarity was destroyed either by replacing the carbon chain with unlinked substituent groups, or by increasing the chain length, herbicidal activity fell away.[3] However, co-planarity was not a sufficient condition to explain the bipyridyls' effects. A second necessary condition was found to be the avoidance of bipyridyls with nitrogen atoms in the 3 or 3′ positions, for these compounds did not produce stable free radicals, and they were consequently ineffective herbicides.

It seemed that the ability of a bipyridyl to be reduced to a free radical was decisive in determining phytotoxic activity. How could this be quantified? A correlation was discovered between biological activity and the ease with which a compound might be reduced, as measured by redox potential. Various bipyridyls were examined to confirm this hypothesis. Diquat with a redox potential of −349mV possessed powerful and rapid phytotoxicity; paraquat, with a redox potential of −446mV was also powerful but slower acting; but salts with redox potential less – that is, more negative – than −500mV either showed no phytotoxicity or were very slow acting. These observations were brought together by assuming that the biological system affected within the plant possessed a reduction potential of −380mV, and by then calculating from this value the free radical concentration in the plant for an applied volumetric dose that just failed to kill it. Whereas the sublethal volumetric dose differed between bipyridyls by a factor of several hundred, the corresponding free radical concentration differed by a factor of only three.[4]

The chemical screen for herbicides had been refined from the general class of quaternary ammonium salts to bipyridylium salts, and within this class to those compounds possessing a particular physical property, a redox potential in a certain range. The value of this work was the increased confidence it gave those responsible for choosing the compounds on which to concentrate. In July 1958, it was possible to predict, from the correlation between phytotoxicity and physical properties, that no bipyridyl superior to 2,2′- and 4,4′-bipyridyls would be discovered. The way was now clear to devote resources to developing these compounds, free from the need to investigate other bipyridyls in the field.

Properties of bipyridyls

Simultaneous with work aimed at elucidating the bipyridyls' mode of action, field trials were conducted to test their uses. The results of the first two years' trials were published in 1958, and characterised diquat as a 'very potent, translocatable, rapid-acting, non-persistent, post-emergent herbicide'. Possible uses were mentioned: destruction of potato haulm, desiccation of cotton and clover, and selective control of weeds in cereals.[5] The uses identified for diquat were matched by important limitations to its effectiveness. Diquat was adsorbed to soil particles, and deactivated very rapidly on contact with the soil. It was also – despite considerable effort to achieve selectivity – non-selective. The importance of these constraints can be recognised by comparing the uses identified in the 1958 report on diquat applications with the survey of world demand for herbicides completed by Plant Protection in the same year. The applications identified for diquat were a small part of the total world market, and would be achieved only if diquat were very cheap. The major world market foreseen, as has already been mentioned, was for a herbicide which possessed both selectivity and persistence – the properties which diquat had now been found not to possess.

Yet diquat was known to be a very potent herbicide, and it seemed reasonable to expect it to have some uses so long as it could be produced at low cost. The cost level required was low. A 1959 survey of future diquat sales assumed consumer prices that varied by a factor of four; the sales estimates varied – inversely with price – by a factor of thirty-five. All the prices considered were at least one order of magnitude below contemporary estimates of likely manufacturing costs for the bipyridyls. The survey of future markets, by identifying such a large gap between what was believed acceptable as a selling price and what was believed achievable as a manufacturing cost, was in no way negative in its effects on the development programme. The size of the identified gap might have lead to the decision to end research. In fact, the effect of identifying this gap was to concentrate attention on closing it. The significance of the market survey was thus considerable and positive, for it provided a clear and paramount objective for subsequent research.

It is useful to consider why this identified gap acted as a stimulant rather than a depressant on research activity. Two reasons may be advanced. First, the research chemists responsible for devising manufacturing routes included some who had experienced at first hand the dramatic fall in price undergone by hexamethylene diamine as it was transformed from being a speciality chemical made on a small scale to a major nylon intermediate, manufactured on a very large scale. And second, the gap between achieved and desired cost was so clearly identified that it acted as a challenge to the ingenuity and scientific skills of chemists. The molecule required, scale of production and cost were all defined. and readily translatable into problems which chemists recognised themselves, correctly, as being well qualified to solve. Experience suggested that the task was both important and achievable, and the stimulus to action was correspondingly great.

The potato haulms on the left were sprayed with 'Reglone' on 14 June 1961. The ones on the right have not been sprayed. The photograph was taken 22 June 1961

The development of paraquat

So far, we have discussed diquat, and have mentioned the 4,4′-bipyridyl only in connexion with its use to confirm chemical hypotheses as to the mode of action of the bipyridyls. The preferred hypothesis suggested that 4,4′ would be highly active. Testing in the field in late 1958 as well as further confirming the hypothesis also showed that the 4,4′-bipyridyl was more effective against grasses than the 2,2′-bipyridyl, diquat. This observation led to trials in Malaya which showed that the 4,4′ compound, later to be known as paraquat, gave better and more persistent control than diquat. The Malayan trials transformed 4,4′-bipyridyl from a supporting role, useful as an adjunct to the 2,2′ by elucidating mode of action, to a compound of industrial significance in its own right; and they opened a new, and more promising, chapter in the bipyridyls' development. Henceforth, diquat was to cede pre-eminence to paraquat.

For paraquat possessed special features which suggested that applications could be developed far greater than those available for diquat, or than those immediately identified in Malaya. Paraquat possessed a fast 'knock-down', followed by very rapid deactivation when the chemical came in contact with the ground and was adsorbed to the soil; it was active against all green soft tissues in plants, but had limited effect against harder tissues such as stems; and it was particularly effective against grasses. These properties were far removed from those sought in hormone weedkillers, but they suggested a new research target. Paraquat offered the opportunity not of replacing existing herbicides, but of extending the use of herbicides by replacing mechanical operations to destroy weeds by chemical control. In particular, paraquat's properties suggested the question: why plough at all, when paraquat could be used to destroy weeds?

The question posed appeared revolutionary, and certainly represented a very large change in the research objective. Further examination showed that a considerable body of previous work supported the inquiry. An experiment conducted in 1944 by which organic material was worked into the top few inches of soil, with hoeing of the surface in place of deep ploughing, had shown that the soil retained a desirable tilth. It had been concluded that the main utility of ploughing was weed control. To those developing paraquat, the further conclusion was apparent that were it possible to remove weeds – as paraquat promised – the plough might be displaced. An extension of the trials showed that 'no plough' techniques depressed yield only of two crops, compared with conventionally ploughed crops. The trial report published concluded with the statement: 'The question of "At what depth must I plough"? may be changed to simply "Must I plough" '?[6]

Although the question was posed in a quite general form, it was particularly pressing in those geographical areas where there were physical barriers to ploughing, either because of rough terrain or because of the danger of soil erosion.

Direct drilling perennial ryegrass with the 3-D drill, into 20 acres of river meadow previously old pasture. Old pasture was sprayed late September with 'Gramoxone'. The photograph was taken 11 April 1969

Experiments had been pursued particularly actively in the Eastern half of the United States, where there were estimated to be 50M acres inaccessible to the plough which would be dramatically improved by chemical tillage. Trials using various chemicals had proved inconclusive, the main problem being to identify a suitable herbicide. A typical report spoke of the need for a chemical to 'quickly and completely kill all vegetation without leaving a residue in the soil which will affect new seedlings'.[7] The properties demanded were those possessed by paraquat, and the demand enunciated raised ambitions in the Plant Protection research workers. These may be seen in the titles of two articles in *Nature:* 'Destruction of pastures by paraquat as a substitute for ploughing' and 'Crops grown using paraquat as a substitute for ploughing'.[8]

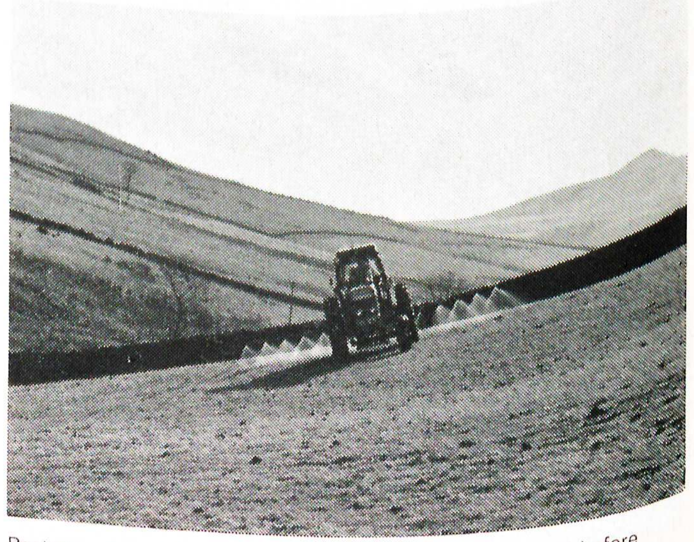
The opportunity thus recognised to develop applications for paraquat widely and imaginatively led to new emphasis on research at Plant Protection, and within an expanding research budget to a dominant place for paraquat. Between 1960 and 1967, total research expenditure grew nearly fourfold. In 1960, paraquat was responsible for 1 per cent of the total in that year, in 1967 for 28 per cent. The rise in expenditure on paraquat was therefore a hundredfold. It mostly occurred after the use of paraquat as a plough substitute became apparent.

The search for manufacturing routes

Both diquat's and paraquat's success depended on suitable and economic manufacturing routes being devised. The challenge this posed, as has been mentioned, was explicit and demanding, and it may therefore be no surprise to find it taken up. However, Plant Protection had no power to direct others to perform the research necessary, and the response to the challenge was not predictable in advance. Yet many organisationally separate parts of ICI became involved, each contributing its own expertise. By June 1957 work to design new processes for diquat was proceeding at Organics Division, at Petrochemicals Division and Plant Protection. Each contributed its own expertise: at Organics, Fielden's original synthetic methods were modified to produce larger quantities; at Petrochemicals, a route based on the reaction between ferric chloride and pyridine was investigated, using the 'hot tube' technology developed at Billingham; at Jealotts Hill, more exploratory work on new routes was performed. None of these research efforts, however, produced an economic route: that based on pyridine, the most promising attempt, was judged unattractive by Organics Division, who would have been – and were to become – responsible for diquat manufacture. At the same time that this decision was made, a further research effort was reported. This, based on observations of effects of pyridine on Raney nickel catalysts made at the University of Adelaide, was the product of ICI (Australia) which took the first steps to develop a route to 2,2′-bipyridyl using Raney nickel.[9] This was reported by Boon to Plant Protection in November 1959; by January 1960 Organics Division had decided to build a plant using the process; and on 30 June 1960 it was reported that they had started operation at a plant built at Huddersfield.

The breadth and commitment of the response to the demand for economic manufacturing routes to diquat need explanation: what convinced so many parts of ICI, each administratively and geographically separate from Plant Protection, to accept the challenge? Partly, we have suggested, the explanation may be sought in the form in which the challenge was communicated, so that it appeared that chemists' skills were relevant. Partly too, it may be explained by the desire of different parts of ICI to participate in what promised – and was promised – to be an extremely profitable venture. The economic factor was an incentive, not a barrier. That this was so owes much to the vigour with which Dr W. R. Boon, Plant Protection's Research Director, promoted diquat in the ICI Divisions. His enthusiasm for diquat, and his determination to find suitable manufacturing routes, overcame geographical and administrative barriers. The role of product champion described by Schon[10] was filled, in developing first diquat and then paraquat, by Boon.

Whereas research on manufacturing routes for diquat was conducted in various centres, that aimed at manufacturing processes for paraquat was performed almost entirely in Mond Division of ICI. Originally, a technological skill possessed by Mond had suggested to Boon that Mond might undertake research. The reaction to Boon's request, made in November 1959, was prompt. By December 1960 work was being done on three levels simultaneously: laboratory experiment, semi-technical plant, and design work for a 50 ton/year plant. This urgent activity displayed by Mond may be explained in various ways: the opportunity for profitable business was an explicit spur; the Mond research department was at a stage when demands on its resources temporarily



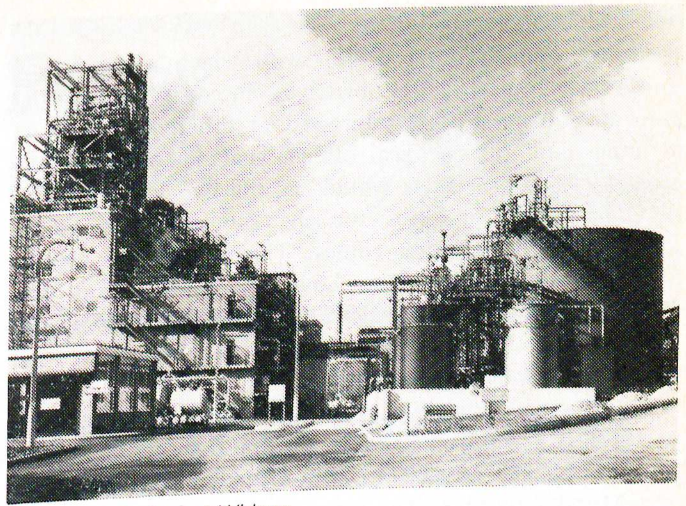
Pasture renewal: spraying old pasture with 'Gramoxone' before renewal with ploughing

lagged behind the resources available, and hence the department contained research workers eager to deploy skills and technologies not fully utilised; and to the explicit opportunity and the available human and other resources should be added the actual people responsible for research and development, who contributed a personal impetus to the project's progress. Two further causes for Mond's urgency may be suggested. First, paraquat's promise would be confirmed – or confounded – only by extensive field trials, which depended on considerable quantities of paraquat being made available from a pilot plant of some size. The project's future therefore could be resolved only if a process adequate for a pilot plant was devised. This was recognised, and added to Mond's urgency. Second, Plant Protection was at this time a subsidiary company of Mond, and could therefore make greater calls upon its parent company to invest in Plant Protection's opportunity for growth than would have been possible with other parts of ICI. Mond in turn was prepared to invest funds in research for new processes at a time when considerable doubts still existed about the product's eventual success. Moreover, substantial investment was necessary before trials could be made to justify the subsequent capital investment decision to build a full-scale plant.

It is helpful to consider the research at Widnes aimed at devising manufacturing routes for paraquat against the concept of research as a means of reducing uncertainty.[11] What were the major uncertainties affecting paraquat's success, and how accurately was the Mond research directed at reducing these? The major uncertainty affecting paraquat in its early development was doubt about the market: in which applications could paraquat be used? What problems of machinery, or of technique, would be encountered? How effective would paraquat be, and hence what price could it command? As has been mentioned, these questions could be answered only by extensive field trials, which could take place only if paraquat were made available in sufficient quantity. The need to produce paraquat for field trials dominated early research at Widnes. The first plant, built in 1961, was erected quickly – it was the first occasion within ICI that a research project was aided by critical path analysis; the process was chemically crude, and not used in later manufacturing routes; the plant operated at flow rates lower, and costs higher, than those originally estimated. Despite this, the pioneer plant operated in 1961 must be judged a success, since it allowed field trials to be performed which reduced the main uncertainty affecting paraquat, the market forecast.

It should be noted that the market forecasts that were made possible by the 1961 field trials did not increase the estimated demand for paraquat. On the contrary, successive market forecasts, based on increasingly reliable information, reduced the estimated total demand. Their value was the greater certainty of the estimates, not the absolute quantity forecast.

Once the commercial uncertainty affecting paraquat had been reduced, the major uncertainty became technological: could paraquat be produced economically and safely on the scale, and to the cost, demanded? To attack this problem a different assault had to be mounted. In mid-1961, a new process was discovered, which by December 1961 had been developed and defined in sufficient detail for capital expenditure on manufacturing plant to be estimated and approved. A plant to make paraquat by this process was operating by the end of 1962. Simultaneously, however, laboratory work



The paraquat plant at Widnes

was going on to explore other potential routes to 4,4′-bipyridyl. This led in turn to a third generation plant – the successor to the pioneer plant of 1961 and to the improved process of 1962. The third process used a route based on reduction of sodium in liquid ammonia with pyridine and solvent followed by air oxidation.[12] This process became the chosen route for the present large scale paraquat plant.

The research on paraquat processes performed at Widnes may be divided into two distinct parts. In the first, the need was to resolve commercial uncertainty about paraquat's outlets, and the Mond research was aimed at devising a process to make paraquat for market trials. Only when trials had been done, and a firm market estimate produced, was the main effort devoted to reducing the commercial uncertainty. The research of 1960 and 1961 differed considerably in aim from that of later years; both, however, were united in having the common objective of reducing the main uncertainty then in existence. Much of the success of the research aimed at new processes for paraquat may be ascribed to the accuracy with which the most pressing uncertainty at any time was first identified, and then attacked.

Discussion

In a previous paper, we have discussed the innovation of the anaesthetic halothane, with considerable emphasis on those factors which affected the acceptance of the new product. The new product required new techniques to apply it in such a way that its advantages were apparent, and these new techniques in turn demanded new equipment. The size of initial investment in equipment, the speed with which improvements could be discerned, and the ease with which the product's advantages could be communicated were significant factors affecting the new product's diffusion – the first hindering, the second and third facilitating, acceptance. The factors which emerged from the discussion of halothane are, we believe, equally important in examining the bipyridyls' diffusion. Rather than explore these in greater detail, however, we have concentrated more upon the internal processes within the originating firm that led to successful innovation. In terms of the model first proposed, we have here been concerned with technology development, not technology transfer.

In examining technology development, we have stressed the need to first identify the major uncertainty, and then instigate research to obtain the information needed to reduce that uncertainty. Much of the success of research connected with the bipyridyls has been attributed in this account to the fact that commercial and technological uncertainties were identified, and to the precision with which research was concentrated on particular objectives. Successive parts of the innovative process were addressed at different objectives: to understand the chemical screens that could be used to predict which bipyridyls would be effective herbicides; to develop widespread uses for the bipyridyls; to devise economic manufacturing routes. Throughout the process of technology development, however, these various sub-objectives meshed together and were integrated so that at any one time the main research effort was directed at what was then the major uncertainty.

It may appear surprising that this was so, since so many parts of ICI were involved in the innovation: Plant Protection Ltd, ICI (Australia), and Organics, Petrochemicals and Mond Divisions. The questions may be raised: 'Why were so many parts of an organisation willing to contribute to the innovation'? and 'How were their contributions integrated'? Answers may be sought in various directions. To the first question, one answer already suggested lies with the skill and enthusiasm of the product champion; another answer is to be found in the economic incentive that stimulated each organisation to seek the opportunity to participate in new and profitable business.

Two other replies may be suggested. The first is the way in which the total problem was subdivided, so that each part of the organisation was asked to make decisions in a business area in which it was already expert. If, following Ansoff,[13] diversification is defined as the combining of a new product and a new mission – a new mission being a customer need not previously satisfied by the firm – bipyridyls did not involve any individual group in ICI in diversification. Because of this, each group was able to place the decisions required in diquat's and paraquat's development in the context of previous decisions. Neither Organics nor Mond Division – the manufacturers – had to take decisions about the merits of rival herbicides, but were concerned with assessing investments in chemical processes, a decision familiar to each. Similarly, Plant Protection – the marketers – was able to relate its decisions on the level of applications research to experience previously gained with herbicides. For all groups, familiarity with the type of decision that was required informed judgement. A second reply, again connected with the industrial organisation, is to be found in the spreading of risk among the various parts of ICI involved. There were two consequences of this. The large research investment in devising manufacturing routes was shared by Organics and Mond, so that the burden was not carried by Plant Protection alone. And each group, although responsible for its own profits, felt that the risk that it individually was incurring was commensurate with the benefits offered. The division of the overall objective between the different groups within ICI was effective, in that each contributed an expertise, and on a scale, appropriate to its resources.

The difficulties of managing innovation within large organisations are often publicised. It is a useful counterweight to consider an example where a large organisation, by effectively using its component strengths and skills, can achieve a complex innovation probably too large, too risky and too difficult to be completed by a smaller company.

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SRC report on 'Colloid science'

As a result of recent developments in experimental techniques and a quantitative understanding of various component problems, the time is now right for significant advances to be made in studies of the fundamental properties and behaviour of colloids. This conclusion is reached by a Panel of the Science Research Council in a report entitled *Colloid science* published on 15 February 1972.

The Panel, which comprised industrial and academic scientists from the traditional disciplines of experimental and theoretical physics, biology and chemistry was set up by the Chemistry Committee under the Chairmanship of Professor G. Allen of Manchester University. Their brief was to survey industrial and academic aspects of research into the science of colloidal dispersions. In addition, as a direct result of a request from the Society of Chemical Industry, they were asked to investigate education and research training given in this area.

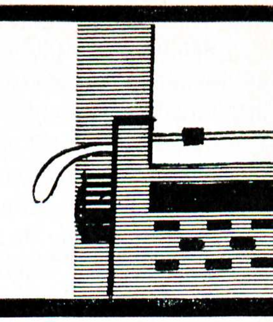
The Panel have summarised their discussions on the present state of knowledge of the chemistry, physics and theory of colloids and its application to biological and industrial problems, and presented a view of the advances which can be expected in the near future and of problems which require urgent attention. Emphasis is placed on the importance of colloid science in industry and evidence is produced that there are far fewer scientists leaving universities and similar institutions with experience of research in colloid science than there are scientists in industry who have to tackle problems in this general area.

A general recommendation is made that the Science Research Council should give special support for research and education in the broad area of colloidal dispersions. The Panel consider that the successful development of the subject will be influenced particularly by the establishment of a special Advisory Panel to monitor progress and coordinate efforts in the field. Such a Panel will organise biennial inter-university seminars aimed at keeping scientists from both large and small research groups in touch with each other and with modern developments at home and overseas.

The report is being presented to the universities and industry before any final decisions are made or funds allocated by the Council.

Colloid science is available free of charge from Mr C. Martyn, Science Research Council, State House, High Holborn, London WC1R 4TA.

Essay review



Taking the rough with the smooth

E R Braithwaite

Dr Braithwaite graduated from the Manchester honours school of cher where he also obtained his master's degree. His doctorate studies at Exe University were concerned with sur chemical aspects of certain solid lubricants. He is a Fellow of the Roy Institute of Chemistry, Fellow of the Institute of Petroleum, and an Assoc Member of the Institution of Mecha Engineers.
Dr Braithwaite's major area of resear in industry has been on solid lubrica he is the author of many papers on this subject, of the book *Solid Lubricants and Surfaces* (1964, Pergamon) and editor of *Lubrication and Lubricants* (Elsevier Publishing).
He is at present European Manager o Chemical Sales and Development fo Climax Molybdenum Company.

'There's a divinity that shapes our ends, rough hew them though we may'.

These lines of Shakespeare were cherished by the boy Acheson and like any other information of interest was stowed away in one of his notebooks. His life, described in this book*, certainly followed such a pattern, for he was so single minded, that he would have been ruined on more than one occasion if he had allowed himself time off from his experimental preoccupations to size up his personal affairs. It is remarkable that this young lad without the benefits of formal education should include Shakespeare in his book list; even more so that he should choose a scientific career and succeed to the extent that the New York Herald Tribune Magazine for 21 September 1930, named Helen Keller, Thomas A. Edison, Orville Wright and Edward G. Acheson as those living Americans 'who have achieved the most'. This judgement was based on four criteria – what he had attempted, what he had accomplished, how difficult was his route to success and how significant to humanity were his achievements.

What then was the scientific and technological climate into which E.G.A. was born?

Dalton had just revised the atomic theory and Wöhler had bridged the gap between organic and inorganic chemistry, though development of the former was delayed by inaccurate atomic weights; Liebig's introduction of combustion analysis coincided with the year of Avogadro's death and Acheson's birth. Indeed the chaotic state of chemistry during this period can be judged from a letter written by Wöhler to Berzelius:

'Organic chemistry just now is enough to drive anyone mad. It gives me the impression of a primeval tropical forest, full of the most remarkable things, a monstrous and boundless thicket, with no way of escape, into which one may well dread to enter'.

Physical chemistry matured finally through Ostwald, when Acheson was fifty years of age, while in the electrotechnical field in which Acheson was also to make lasting contributions, Volta was inventing his batteries and Davy doing his classical work on electrolysis. Faraday ruled supreme at the Royal Institution.

In the light of this brief period sketch, Acheson's theories of the formation of silicon carbide and graphite and masterful use of the new electrical toy appear all the m remarkable. Acheson was one year old when the first was prepared by Faraday and five years old when the t 'colloid' was introduced by Thomas Graham though had reached his half century before 'colloids' becam respectable branch of science.

During this great period of scientific rebirth in Eur modern America was being born; Abraham Lincoln President when Acheson was one year old and the Civil started when he was five. Little wonder that his text bo were his own jottings and his early tutors after only th years of schooling were railroad men, miners and iron work

Acheson was born in Washington, Pennsylvania in 18 He was the son of a grocer who eventually moved neare Pittsburgh where he became a furnace man at the Montic Furnace Company which was liquidated when Acheson seventeen, the year his father died. It is my personal feeli not expressed by Szymanowitz or others, that the you Acheson must have been encouraged by his father duri these early years to aim high; certainly his scientific intere were nurtured at the iron works. The next few years of life were occupied with various menial jobs as would expected of a relatively uneducated impecunious you who had lost his father. After clerking, working on the ra way and doing various jobs on a small oilfield, one of t

* 'Edward Goodrich Acheson – a biography', 1971, Raymond Szymanowitz, pp 628, *New York: Vantage Press*, $10·00